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Art. I.—ON THE HYDRATED PEROXIDE OF IRON, AS AN ANTIDOTE FOR ARSENIC.

BY ROBERT B. HALL, M. D.

Princess Anne county, Virginia, Aug. 6, 1838.

Professor Dunglison.

The hydrated peroxide of iron has, for some time past, been engaging the attention of the medical world as an antidote for arsenious acid. Much contrariety of opinion exists upon the subject, and while many of the most prominent members of the medical corps exhibit a spirit of scepticism with regard to its anti-arsenical virtues, still, a host of names, "not unknown to fame," are disposed to consider its properties in this point as specifical. The object of this communication is to sum up, in a concise manner, the most positive testimony in favour of its properties as a counter-poison, which my limited opportunities have enabled me to collect, and at once submit it to the medical world, together with a few experiments of my own, in order that some of your numerous readers, who feel interested in the subject, may, by comparing experiments, or otherwise, incontrovertibly establish an important point in toxicology.

In the Bulletin Générale de Thérapeutique, for December, 1834, MM. Soubeiran and Miguel, of Paris, have published their experiments with the supposed antidote upon the canine species. The acid was first given, with the view of testing the anti-arsenical properties, said to be inherent in the economy of the dog family. The administration of large doses of the poison produced copious vomiting, and in every instance the animals submitted to their experiments recovered, without any other pathological symptom; it therefore became necessary to tie the œsophagus after the introduction of the arsenious acid, to obviate vomiting; the consequence of this step was the uniform death of the dogs in from two to four hours. The third step in the experiments consisted in giving a certain portion of the poison, followed by twelve times its amount of the hydrated tritoxide of iron; and an immediate closure of the œsophagus by ligature,—life was prolonged to seventy-eight, eighty-four and ninety hours. It must be here remarked that tying the œsophagus is a fatal operation, and generally destroys life in from sixty to one hundred hours. Consequently the salutary effects of the iron cannot be doubted in these cases, for where the arsenic was given, and the ligature tied, *without* the administration of the antidote, the longest period of sufferance was four hours. Several other experiments were instituted to determine how long after the admission of arsenic into the system the antidote may be given with a reasonable hope of success; the results all tended to prove the value of the iron, and the experimenters emphatically conclude that it is a counter-poison where arsenious acid has been swallowed.

Drs. Borelli and Demaria, two physicians of Turin, have published in substance the following:—

Experiment 1.—Twelve grains of arsenious acid given to a dog; the œsophagus tied, but no peroxide of iron administered. Result, death in three hours.

Experiment 2.—Nine grains of arsenic given to a dog, and immediately after three ounces of hydrated tritoxide of iron, the œsophagus tied; seven hours afterwards the ligature was loosened to introduce another dose of the antidote. The animal lived ten days, and was then killed for a necroscopy.

Experiment 3.—Fourteen grains of the poison, given to a large dog, and the œsophagus tied; it was loosened half an hour afterwards to introduce one ounce of the iron. The dog perfectly recovered, and was killed some days subsequently, by a dose of arsenic administered without the antidote.

Conclusions.—Four and a half parts of the iron necessary to neutralise one of arsenious acid. The hydrated iron loses its virtue by age; if *fresh*, it is as certain an antidote to arsenious acid as albumen is to corrosive sublimate.

In the Journal de Médecine et de Chirurgie pratique for September, 1835. M. Geoffroy, treated a hair-dresser, who had taken an ounce and a half of white oxide of arsenic. He gave ℥ vi. ss. of hydrated peroxide of iron, in divided doses. The patient recovered. Conclusions,—the iron an antidote; it also produces a brisk catharsis, independent of its counter-poison properties. The iron should be freshly prepared.

In the Journal des Connaisances Medico-chirurgicales for November, 1835. MM. Bineau and Majesté, of Saumur, attended five little girls, who had eaten of cake, containing arsenic, prepared for the purpose of destroying rats; from four to six ounces of iron was given to each child with the happiest results. Conclusion,—the hydrated peroxide of iron a certain antidote for arsenic.

Professor Von Specs, of Vienna, has recorded some experiments in a German Journal, which have been transferred to the pages of "Bell's Library," for January, 1838. Results satisfactory.

In Professor Dunglison's "Medical Intelligencer," vol. 2, No. 1, Dr. Joseph E. Muse, of Maryland, has published his experiments with iron upon a dog. Result unsatisfactory—the animal drugged largely, but vomited in the early part of his experiment and recovered. Conclusion,—the animal preserved, not by the tritoxide of iron, but by the peculiar anti-arsenical properties resident in the animal economy of the dog.

Dr. Richard H. Thomas, of Baltimore, has published in "Dunglison's Intelligencer," vol. 2, No. 8, an account of a case in which he treated an individual who by mistake swallowed twenty grains of arsenic, with the hydrated iron, followed by magnesia. A perfect cure was effected. It is not positively proven that this is a genuine case of poisoning by arsenic, but from the facts connected with the case, there is every reason to believe that the arsenious acid had been swallowed.

I have collected some of the above cases from the "British and Foreign Medical Review," to which I would refer those who are interested for a more enlarged description of the numerous experiments. I have been unable to procure but one of the foreign journals to which I have referred (La Bulletin Générale de Thérapeutique), but I have no doubt that the translations given by Drs. Forbes and Conolly are faithful.

EXPERIMENTS BY THE AUTHOR.

Experiment 1st. Twenty grains of arsenious acid given in clear water to a large dog. One hour after, gastric distress; vomiting and perfect recovery.

Experiment 2d. Same dose given, and œsophagus tied; death in two hours and seven minutes.

Experiment 3d. Three and a half grains arsenic given to a small dog, œsophagus left untied; in two hours much derangement of stomach, but no emesis. Death occurred in four hours and forty-six minutes.

Experiment 4th. Twenty grains of arsenic given to a dog; œsophagus tied. One hour after, one ounce of the hydrated peroxide of iron, mixed with a half pint of water, was thrown up the rectum. Death in sixty-six hours.

Experiment 5th. Twenty grains given to a dog, followed by one ounce of the antidote; œsophagus tied; catharsis and recovery. Conclusions,—arsenious acid innocuous upon the economy of the dog, when given in doses sufficiently large to produce vomiting; if the poison be retained either by ligature or by doses too minute to excite emesis, it is certainly fatal. The hydrated iron *is* an antidote.

The antidote was procured after Bunsen's mode—℥ i. of iron filings is thrown into ℥ viii. of aqua regia, and heated gently in a glass vessel; after the iron dissolves, ℥ xvi. of cold water are added, and the metal is precipitated by adding ℥ iii. aq. ammoniæ. The whole is now agitated and filtered, and the powder dried in the shade for use.

Respectfully your obedient servant,

Robert B. Hall, M. D.

Art. II.—PHILADELPHIA HOSPITAL (BLOCKLEY).

DR. DUNGLISON, ATTENDING PHYSICIAN.

Case of Hemiplegia of the Left Side—Local Softening of the Right Hemisphere—Ossification of the Aortic Valves and of the Arteries of the Extremities Reported by Edwin A. Anderson, M. D., of Wilmington, N. C., and Alexander M. Vedder, A. M., of Schenectady, N. Y., Senior Resident Physicians.[1]

James Brackenbridge, æt. 63, entered Men's Medical Ward, No. 3, July 4th, 1838. Is a native of Scotland, but has been in America forty-three years: a weaver by trade; married. The following information was obtained from his friends:—Has been subject to slight cough for four or five years. On the morning of the 27th of June, it was observed that he was covered with an eruption resembling measles. On that day he took his breakfast, and felt quite as well as usual. Had a large amount of work on hand, and exerted himself more than usual at weaving. At noon felt unwell, and took a light meal. After dinner, while weaving, he fell down insensible, but recovered his consciousness in a few minutes. Was attended by a physician, who prescribed wine whey and "powders." Had been subject to what he termed a "great heat in his head." No vomiting; no cephalalgia previous to the attack.

At his entrance he was sent to the Surgical Ward by mistake; was then pulseless, and could not speak. Stimulants were administered, and a blister was applied to the nape of the neck. Reaction followed in a few hours.

Present state, July 4th, 1838.—Emaciation considerably advanced. Moderate sized frame. Decubitus dorsal; expression cerebral; lies dozing, with his eyes half closed, but can close them; conjunctiva not injected; pupils natural and equal; the right nostril dilates in respiration, the left does not; mouth drawn to the right side, the left angle depressed; tongue can be protruded, the point decidedly drawn to the left side; speech very thick and slow; no difficulty in swallowing; tongue coated, yellow; intelligence and senses obtuse; memory very feeble; no cephalalgia; no vomiting; appetite not lost; complete paralysis of the extremities of the left side; sensibility of that side diminished; extremities cool; head warmer than the other parts

[1] This is the case, the report of which was promised under *Hemiplegia*, No. 2, in the "Intelligencer," for Aug. 15, p. 156.—*Ed.*

of the body; passes his urine *guttatim* in bed; no rigidity of paralysed limbs; the arteries of fore-arm are ossified, and the femoral seems to be in the like condition; pulse 84, feeble and small.

Heart.—Impulse feeble; a strong *bruit de scie* or saw sound, synchronous with the first sound, having a ringing character; the second sound is roughened; the abnormal sound can be heard throughout the chest anteriorly.

Prescription.—A blister to the nape of the neck, to be kept open.

R. Ammoniæ carbonatis ℥i.; Ft. emuls. ℥vi. Sumat cochleare magnum secundâ quâque horâ; wine whey.

N. B.—Has a sloughing ulcer on the right trochanter.

July 5th.—The pulse having risen last evening the ammonia was discontinued. Patient is now more gay; asks for a pipe to smoke. Slept well. Still passes the urine as before. A thermometer, placed under the tongue, rises to 100° Fahrenheit. Pulse 84, feeble.

Continue wine whey.

July 8th.—Less lively than on the 7th; speech still slow, a little less thick; sleeps badly, is delirious at times; passes his urine freely; bowels not moved except by an enema; strength as before; pulse more feeble.

Continue the whey.

July 13th.—Intelligence clearer; memory improved; strength increased; sleeps well; appetite good. The ulcer has assumed a healthy appearance; no cephalalgia; no pain in the extremities; pulse feeble, 72.

Continue the wine whey and let him have good diet.

July 20th.—More feeble; sleeps badly; complains of pain in his limbs; appetite lost; pulse more feeble, and can scarcely indeed be felt.

He died on July the 26th.

Necroscopy, ten hours after death.—Exterior: emaciation about equal on both sides. No rigidity of either side.

Brain.—The dura mater was thickened, and strongly adherent to the whole surface of the cranium. The arachnoid was of a light slate colour, thickened and clotted with a few white points. Considerable effusion of serum beneath it. It could be readily detached from the substance of the brain. That portion covering the lateral superior and posterior portion of the posterior lobe of the right side was finely injected, and contrasted strongly with the surrounding membrane. The large veins were distended with dark blood. The substance of the brain was of a darker colour than usual. Convolutions remarkably distinct and of good consistence; the medullary portion presenting a few red points. The lateral ventricle of the left side was distended with a slightly opaque serum, about an ounce in quantity. Plexus choroides pale. On removing the substance of the brain on the right side, to the depth of an inch and a half from the summit, a space occupying the centre of this side was found of a reddish colour, much softened, and almost semi-fluid. This pulpy condition extended from within half an inch of the falx to the external portion corresponding with the central part of the parietal bone, including a space of about four inches in diameter. The portion of the brain involved was about the size of a large orange, and resembled, in consistence and colour, the contents of a scrofulous abscess. The tissue was completely broken up. The cortical portion involved could not be distinguished from the medullary. The right ventricle was not involved, and presented a natural appearance. The plexus choroides was of a darker hue than on the left side. The corpus striatum seemed not to be involved in the softening. That part of the brain beneath the ventricle was of good consistence. Cerebellum normal.

Thorax.—The right lung presented externally, in its lower lobe, a dark appearance, and was engorged with blood; the tissue was easily broken up, and contained air. The upper lobes were of a light gray colour and supple; no engorgement or tubercles. The left lung was strongly adherent to the costal pleura by several old bonds of union. The same appearance existed

in its lower lobe as on the right; upper lobe supple. Tissue of good consistence.

Heart.—Externally, the right ventricle, in a space of two inches in diameter, was of a white colour. Three other spots, about the size of a shilling, were found on the left ventricle. The left ventricle was three fourths of an inch thick (hypertrophied.) The semilunar valves of the pulmonary artery were soft and flexible. The aortic valves were irregularly ossified, except at their free margins. The ossific deposites were of an irregular shape, angular, and varying in size from that of a grain of wheat to that of a mustard seed—each valve containing fifteen or twenty. The valves were rigid and could not close perfectly. The ossific deposites were on the surface next to the aorta, and were scarcely covered with a membrane.

Half an inch below the valves a deposit of the same character was found, of about the size of a grain of corn. The auriculo-ventricular valves were not ossified. The aorta presented a few roughened points.

The external iliacs, commencing at the bifurcation of the aorta, were ossified; the ossification extending to the termination of the arteries. This ossification was so complete as to prevent collapse of the vessels, and to allow of tubes of solid bone, half an inch in length, being removed. The calibre seemed much diminished in size, the femoral artery admitting with difficulty an ordinary silver female catheter. Commencing at the subclavian artery, the same condition existed throughout the superior extremities. The carotids were not ossified.

Abdomen.—The stomach and small intestines were normal. The large intestine contained hardened fæces. Abdominal viscera generally healthy. The bladder was distended with about a pint of urine.

E. A. Anderson,
A. M. Vedder.

Art. III.—ON WIND-CONTUSIONS.

(In reply to Dr. Annan, of Baltimore.)

BY JOHN R. PURDIE, M. D.,

Smithfield, Va., Aug. 25, 1838.

In publishing my paper of the 24th May, it was not my object to be drawn into a discussion of the validity of my own or any other person's opinion respecting the cause of "wind-contusions;" neither was it my design to extort from any one praise for "the disposition I have manifested to cast light upon" a "dark subject;" (for this compliment, however, Dr. Annan has my kindest acknowledgments;) but simply to perform an act of justice to one, at least, of the greatest teachers of chemical philosophy our country has ever possessed. It is true, that the opinion of Dr. De Butts conflicts with that of every author I have read on this subject; and it may seem extreme boldness in me to attempt to sustain it, thus opposed by this great array of authority. But as "by the collision of opinions truth is elicited," I will, in a concise and candid manner, lay before your readers such facts as seem to me most forcibly to support this one; firmly convinced that science cannot be injured by the result.

I do not wish to occupy the critic's station, yet it may be necessary, in order to sustain my own opinions, to pass some parts of Dr. Annan's paper through a critical notice; and reserving for another place any remarks on the long list of mistakes which the doctor has so exultingly charged me with, I will take up his strictures on Dr. De Butts's "hypothesis." Then, the first assertion to be noticed is contained in the following sentence,—"We know that friction and chemical action are powerful agents in exciting

the phenomena of electricity, but we have no evidence that an appreciable amount of this influence is called into action by the friction of cannon-balls and shells against the particles of the atmosphere."

In this passage Dr. Annan has committed an error equally as much, if not more, to be "regretted," than the one with which he charges me, in the first paragraph of his paper. Mine simply consisting in neglecting to "acquaint myself with the minute details" of a case, when the principle only was required—his is an incorrect statement of the language which I used, and changing it to suit his purpose. I said, that "many bodies in passing through the atmosphere *collect* electricity;" and again, "that bombs *collected* a sufficiency of this fluid to become luminous in the dark;" but nowhere have I asserted, that "the *friction* of cannon-balls and shells against the particles of the atmosphere called this influence into action."

But, for the sake of argument, admit that I meant friction; and then let us examine if any evidence can be found to prove this position, the truth of which the doctor so boldly denies. If the evidence of the senses of seeing, hearing and feeling, can establish a fact, they can all be brought to testify, that not only an "appreciable," but a large amount, of electricity is "called into action" by *friction* between metals and atmospheric air. The ancients, as well as M. Rozet, witnessed numerous electrical phenomena which go to prove this fact. Seneca says, that "during the night before the battle which Posthumius gained over the Sabines, the Roman javelins emitted a light like torches." According to Procopius, the same appearance was seen in the war which Belisarius waged against the Vandals. Livy, Plutarch, Pliny and Cæsar, all mention similar facts, and the two last observed them. That these facts should escape the notice of the merely medical reader is not astonishing; but I did not suppose, that the numerous and interesting experiments and observations which have been made on the spontaneous electricity of the atmosphere, could be unknown to any one who has passed through a full course of medical studies. Beccaria, De Saussure, Cavallo, and many other philosophers, after a great expenditure of time, have laid the results of their labours before the world. And how can we explain the phenomena produced by their electrical kites, and other atmospherical electrometry, unless we admit that metals when insulated in favourable situations possess the property of collecting electricity. The fluid is there,—we can see it, hear it, and feel it; it escapes but slowly through the conducting medium of the atmosphere, but from a direct communication with the earth through the human body the effects are serious, and have sometimes been fatal.

But it may be said that electricity thus collected was attracted from thunder-clouds by pointed rods. In the melancholy case of Prof. Richman, of St. Petersburgh, it might have been so—it is admitted by all, that the electricity of the atmosphere is most abundant during the presence of thunder-storms. Mr. Andrew Crosse, in his "table of the different states of the atmosphere in which electricity appears," states it to be most powerful during the occurrence of regular thunder-clouds. Other observers make the same remark; but not one hints at the idea that this superabundance is the effect of attraction from the clouds. The very apparatus with which Mr. Crosse experimented, and from which he drew his conclusions, was a copper wire, extending *horizontally* between two posts, and did not communicate with any vertical points. From this we infer, that the electrical condition of the atmosphere varies considerably under different circumstances, and that the presence of thunder-clouds exalts this condition—but there is no evidence that the fluid thus manifested by the electrometer is drawn from the clouds.

One of the most remarkable examples of excess of this fluid in the atmosphere, was observed by M. Cavallo when there was neither thunder nor lightning; neither had there been any for three days before or after the observation. Its length necessarily prevents me from giving it *in extenso*.

I must therefore content myself with stating such portions as bear most forcibly on the question under consideration. On the 18th Oct., Cavallo raised his kite with three hundred and fifty feet of string, the end of which was insulated and otherwise prepared for experimental observations. Very soon he was enabled to charge coated phials and give shocks. On the approach of a large black cloud the electricity decreased, and shortly became imperceptible. Soon it showed itself again, having changed in the *interim* from positive to negative; and the electrometer stood at 15°, which is pretty strong, and Cavallo again charged vials and gave shocks, but in a short time the electrometer arrived at 35° and was still rising. He became somewhat alarmed at this rapid advance, and determined to remove the insulation of the string, in doing which, he says, "I received about a dozen or fifteen very strong shocks, which I felt in my arms, breasts, and legs, shaking me in such a manner that I had hardly power enough to effect my purpose, and to warn the people in the room to keep their distance." When he let go the string, a snapping began between it and the window-shutter, which was heard some distance from the room. A gentleman who assisted him in pulling the kite, came near letting the string go, from shocks which he received, although their severity was much diminished, (see Phil. Trans. 1776, p. 407, and 1777, p. 48.) During this rapid collection of electricity no lightning was seen or thunder heard; and although we must conclude that it was not a thunder-cloud, still, it produced a great effect on the electrical condition of the atmosphere, the *rationale* of which I will not attempt to explain.

How greatly these effects were increased during the passage of thunder-clouds, may be seen by referring to the observations of M. Dalibaud, in 1752, of M. Romas, in 1753, and the fatal experiment of Prof. Richman above alluded to. M. Romas, during the approach of a cloud, from which some thunder was heard but no lightning seen, raised his kite, and from the end of the string drew sparks more than an inch long, at the distance of six inches; which after a while were converted into "flashes of fire about a foot long," and accompanied with a noise which "was heard at the distance of more than five hundred feet." He also observed straws, one of which was a foot long, "dancing between the ground and the white iron tube, at the junction of the string and silk cord, three feet distant from the ground." "The longest straw was attracted by the iron tube, and immediately he heard three loud noises," which "were heard in the centre of the town (the experiment was made out of the town);" and this straw "followed the string of the kite, and was even seen at the distance of forty-five or fifty toises, going with great rapidity, being sometimes attracted and sometimes repelled, and every attraction being accompanied with long plates of fire, attended with continual explosions."—"After the first spontaneous explosion, till the end of the experiments, there was no lightning, and almost no thunder."

Professor Richman was killed while experimenting during the passage of a cloud which was much more highly charged than this last. Shortly after a tremendous clap of thunder, the professor "inclined his head to the gnomon" of the electrometer "to see the degree of electricity indicated; and when he was in that bent posture, with his head about a foot distant from the rod, a large globe of white and bluish fire, about the size of M. Sokolow's fist, flashed from the rod to his head, with a report as loud as that of a pistol."

These facts prove that thunder-clouds possess the property of greatly exalting the electrical condition of the atmosphere, other clouds produce this effect in a lesser degree; and moreover, the experiments of the same philosophers which I have mentioned, show, that under any and every circumstance, atmospherical electricity is to be found. Signor Beccaria states as the result of his observation, that "the electricity was always perceptible in a *clear* sky and calm weather." M. De Saussure observed, that "in cloudy weather, when it is neither rainy nor stormy, the electricity

follows nearly the same laws as when the weather is serene," and that "with the exception of stormy weather, the electricity of the atmosphere is strongest during the prevalence of fogs," at which time, he says, "the sky is clear on the mountains," and "the fogs conduct to the earth the electricity of the serene air which reigns above them." M. Cavallo says, "The air appears to be at all times charged with positive electricity," and "the electricity of the kite is generally diminished by the presence of clouds;" and in another place he says, "The electricity is always weakest when it is cloudy and warm, and about to rain."

We have seen horizontal and vertical metallic rods, as well as kites, collecting this powerful agent in quantity sufficient to be not only appreciable by the senses of seeing, hearing, and feeling, but also to produce serious, even fatal results. Its presence is evinced under every different condition of the weather, whether clear, cloudy, or stormy, differing only in amount; one state of the weather being more favourable to its abundant diffusion than another. With such facts before us, if our imagination is too weak to conceive that cannon-balls and shells when insulated possess the same property, the principles of *induction* will force us to that conclusion. It is true, that in the one case the air strikes against the rod or kite; while in the other the ball is forced against the air—and if this varies the result, it can only be as to the quantity collected in the same time. Its great rapidity favours this idea; for it is well known, that the faster the cylinder of the machine revolves, the greater the quantity collected in a given time. With this view the multiplying wheel was invented. Also, in that remarkable observation of M. Cavallo on the 18th Oct., the wind was rather strong, and Mr. Crosse and De Saussure both say the electricity is strongest in stormy weather. From which we may infer, *cæteris paribus*, the more rapidly the particles of air come in contact with the metal, or *vice versa*, the greater the collection of electric fluid.

In these remarks we have supposed that this "influence" was "brought into action" by "friction," and perhaps this hypothesis is as plausible as any other. We do not know in what manner clouds or other substances become charged in their passage through the air; and if Dr. Annan chooses to deny that *friction* has any agency in producing this effect, he can do so perhaps as safely as if he were to deny any other hypothesis on the subject. It is a fact incontrovertibly established by various philosophers of the past century, and by the observations of men before and since the commencement of the Christian era, that metals and some other bodies, when insulated, (and sometimes even when they are not insulated,) collect electricity from the surrounding atmosphere. How it is collected, whether by *friction* or otherwise, is mere hypothesis; it is forbidden ground, on which I will not tread. But if the doctor is more fond of one *rationale* than another, I ask him for the reason of his preference.

In the case of General Elliott, Dr. Annan exultingly asks, "Where was the electricity?" It was never imagined that every ball and shell collected a sufficiency of this fluid to produce fatal effects; so far from it, we have shown by the experiments heretofore alluded to, that large quantities were gathered on some occasions, while on others it was scarcely perceptible. Possibly the weather on that day was very unfavourable to the generation of atmospheric electricity; and I offer this as a reasonable answer to the doctor's query.

Dr. De Butts is charged with "a great oversight" in "mistaking the light from the burning fuse for an electrical phenomenon." Perhaps this may be correct. But what charge can be made against Posthumius, Procopius, Pliny, Cæsar, and M. Rozet, who witnessed this appearance entirely unconnected with the act of combustion? The light in those instances cannot be regarded as the effect of "burning fuses;" and if a halo of light has been seen around the points of javelins, spears, flag-staffs, and even the human fingers and hair, is it very difficult to suppose the same

appearance might not be seen around cannon-balls under similar electrical states of the atmosphere?

Fish are frequently killed when guns are discharged at them in the water, without showing any wounds, by what the doctor very significantly terms *water-concussion*. The very serious effects produced on a late occasion on two young men, while *under* the water, by the firing of a cannon from the Battery, New York, were of a similar nature. We can readily conceive that when water is acted on by strong vibrations of air, its superiority as a medium for transmitting vibratory motions will cause the most unpleasant effects, provided the brain is exposed to the concussion. Such is the case with fish or other animals. But I will defy Dr. Annan to adduce a single case of *water-concussion*, either in man or beast, fowl, fish, or amphibium, unless the great nervous centre was immersed; and without such proof I must conclude the frog was killed by some other agency. As to concussion of air killing an animal, I have had no evidence of such a fact. The organ of hearing is sometimes seriously injured by the discharge of heavy artillery, but instantaneous death has never been produced by such a cause, so far as my knowledge extends.

Ten years have elapsed since the case of Lieut. Claggett was related to me, which will sufficiently account for any ignorance in its minutiæ which I have evinced. Moreover, the exact position of the unfortunate officers who were present; how many were injured; &c. &c., were entirely forgotten in considering the more important fact, that no wound was found on his body. But let us examine the bill of mistakes brought against me by Dr. Annan, and perhaps they may be made to assume a less glaring appearance. The first and second on the list may be examined together; in them he charges me with saying "the lieutenant and sergeant were at the same gun," and from the following words, viz. "The sergeant was killed, but not at the same gun;" leaves the reader to infer that I had made such a statement. Grammarians make some difference between the articles *the* and *a*. If there was no non-commissioned officer at the gun, it was a deviation from military custom; he may have been *a* sergeant or *a* corporal, but I'll warrant one or the other was there. But so far from saying *the* sergeant was killed, I was not aware that any such officer was killed, and my words cannot be contorted into any such meaning. I simply "thought, perhaps, others were injured," but as to the sergeant's being killed, this deponent has said nothing.

From one remark of my informer, I have a pretty clear recollection that he said the bomb burst in the air. The remark was, "that it appeared to rain iron." And that he stated "no bruise or scratch was found on his body," I am perfectly satisfied. This last made the impression on my mind, and it was the more interesting, as I received it from an eye-witness.

Blows on the epigastrium even with the fist, or kicks from a horse, have occasionally produced immediate death. Falls on the abdomen may result in a similar manner. But I have never heard of an animal's being seriously injured from blows, kicks, falls on the thorax, without having the skin lacerated or ribs broken. If Dr. Annan can believe that a twenty-four pounder could fall on a human being and "crush him," without breaking bones or tearing skin, he has more credulity than I possess. There is no circumstance connected with this case which leaves such an impression on my mind; or that he was struck by any piece of metal. What are the *post mortem* appearances of persons killed by lightning? They may not always be alike, but in a case which occurred on the 6th inst., and reported by Dr. John B. Brown, for the Boston Daily Advertiser, he says, "A considerable quantity of blood has been discharged from the ears and nostrils. About three ounces of blood was found on the outside of the cranium, between that and the scalp; and on the inside of the cranium, between that and the *dura mater*, eight ounces of black blood were found extravasated—*black as ink*." The cranial contents of Lieut. Claggett were not examined, but the

external appearances of the body correspond precisely with this case of Dr. Brown; except that, in one the shock was communicated to the breast and neck, and in the other to the head, which accounts for the difference in the locality of the extravasation.

In conclusion I will simply observe, that from the absurdity of considering the elasticity of the abdomen and thorax sufficient to cause a ball or bullet to rebound; and from all the facts derived from the experiments and observations of philosophers on the laws of electricity, I am compelled to reject the former, and adopt the electrical explanation of *wind-contusions;* and if Dr. Annan is led to a different conclusion, I am satisfied it is an honest difference of opinion.

JOHN R. PURDIE, M. D.

BIBLIOGRAPHICAL NOTICE.

Allison on Lymphatic Hearts.[1]

In the year 1833, Professor Müller, of Bonn, published in the Transactions of the Royal Society of London an account of a discovery he had made, that the frog and several other amphibious animals are provided with large receptacles for the lymph, situate immediately underneath the skin, and exhibiting distinct and regular pulsations like the heart. The use of these lymphatic hearts appeared to be to propel the lymph along the lymphatics. In the frog four of these organs were found, two posteriorly, situate behind the joint of the hip, and two anteriorly on each side of the transverse process of the third vertebra and under the posterior extremity of the scapula. The pulsations of these lymphatic hearts did not correspond with those of the sanguiferous heart, nor did those of the right and left side take place synchronously. They often alternated in an irregular manner.

These researches of Müller suggested to Dr. Allison a farther investigation of this subject, the result of which is the interesting paper before us,—interesting not only for the information it contains, but for the promise of farther investigations on the part of its zealous author in the important domain of zootomy. Dr. Allison has traced these pulsating organs in the tadpole, the frog, and in certain of the sauria, ophidia, and chelonia. He states farther, that since his paper was in type he has discovered the lymphatic hearts in fishes, namely, in the sunfish, catfish, perch, &c.

From his observation on the pulsating organs of the tadpole and frog, Dr. Allison infers,—

"*First.* That the pulsations of the lymphatic organs vary in different specimens from 60, or less, to 200 per minute.

"*Secondly.* That they vary in the same individual so as sometimes to double themselves in frequency.

"*Thirdly.* That the lymphatic pulsations bear no fixed relation to those of the pulmonary heart or to respiration, the lymphatic hearts being on an average of greater frequency."

[1] Observations Relative to Lymphatic Hearts. By Joseph J. Allison, M. D. (Extracted from the American Journal of the Medical Sciences, for August, 1838.) 8vo, pp. 19. Philadelphia, 1838.

Copland's Medical Dictionary. Part V.[1]

This part, which is smaller than its predecessors, and has been long in preparation, contains Diseases of the Heart, Herpetic Eruptions, Hiccup, Hooping-Cough, Hydatid, Hypertrophy, Hypochondriasis, Hysteric Affections, Jaundice, Ichthyosis, Impetiginous Affections, Impotence and Sterility, Indigestion, Induration, and Infection.

The fifth part exhibits learning and industry—although the former admits of being yet more widely extended. The chief fault of the entire work, we think, is, that there is too much attention to details, and too little to principles.

We have been applied to, by the way, more than once, to know if we would not publish these numbers of the dictionary as they appear, to complete the sets of those who subscribed to a former periodical, established on the same plan as our own. Slight reflection would have shown our correspondents that were we to do so, we should be furnishing only the remnant of a book to the large mass of our subscribers.

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The American Phrenological Journal.[2]

This is the title of a new monthly periodical, issued from the same office as the "American Medical Library and Intelligencer," and therefore, we need hardly say, well "got up"—to employ the language of the craft.

The articles are six in number,—1. Introductory Statement. 2. Review of Sewall's Examination of Phrenology and of Caldwell's Phrenology Vindicated. 3. A Phrenological Analysis of Conversion. 4. Pathological Fact Confirmatory of Phrenology. 5. Phrenology in Germany, No 1. And 6. George Combe, Esq.

This first number exhibits power and energy on the part of the editor, and we doubt not that such will be the general impression.

The price to subscribers is $2 per annum.

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American Journal of Homœopathy.[3]

The object of this journal is avowedly to maintain the doctrines—if they may be so termed—of the homœopathists; but we do not think, from this specimen number, that the editor conducts a feeble cause successfully.

Uncertain as are the facts, and idle as are most of the hypotheses—for theories they can scarcely be called—they might, by the exercise of more ingenuity, be so framed as to make a greater impression on those for whom they are especially directed. On these subjects, however, we presume that but few homœopathists will agree with us.

The appearance of this journal will be an additional evidence to our brethren on the other side of the Atlantic, that there are always too many of us who are disposed to adopt any opinions from the practical exercise of

[1] A Dictionary of Practical Medicine; comprising general pathology, &c. &c. By James Copland, M. D., F. R. S., &c. &c. 8vo, pp. 143. London, 1838.

[2] The American Phrenological Journal and Miscellany, with various mottos. 8vo, pp. 32. Philadelphia, 1838.

[3] The American Journal of Homœopathy, edited by an association of homœopathic physicians. Vol. I., No. 1. August, 1838. 8vo, pp. 36. Philadelphia, 1838.

which profit is to be derived. In a letter now before us, dated Dublin, July 23d, 1838, and written by a gentleman who is admitted to be one of the most eminent living teachers and writers our correspondent remarks,—"I find that the Americans have been visited by the epidemic pests of animal magnetism, homœopathy, phrenology. Why do you not, in revenge, inflict Thompsonianism, or some absurdity of American growth, on us?"

Composition of the Notorious Quack Liniment of St. John Long.[1]—Dr. Macreight called the attention of the Medico-Botanical Society to the composition of the lotion employed by the notorious St. John Long, which, at one time, excited no small controversy with respect to its imputed virtues. It was known to most of the members, that at the death of that person, who, like Paracelsus of old, died with a bottle of his elixir-vitæ at his bed side, the secret of the composition was sold for 10,000*l.* The present proprietors had now exposed it for sale, and it was in the power of any person to examine it. He (Dr. M.), assisted by his friend, Mr. Fownes, had analysed it with some care, in Mr. Everitt's laboratory, and the result was, that it consisted of oil, of turpentine and acetic acid, held in suspension by yolk of egg. Having satisfied themselves that no other acid was present, they found, by saturating with carbonate of potass, that the quantity of pure acetic acid was 5.3 per cent. They then distilled over the liquid portion, and found there floated on the surface, oil of turpentine to the amount of 25 per cent.; the remainder of the liquid had the taste of vinegar, but was without any pungency or bitterness. The residual mass, also, which was 7.5 per cent., when quite dry, was void of any stimulant or bitter taste; this on burning gave out the odour of animal matter. He need not tell the gentlemen of that society how difficult was the analysis of most animal or vegetable matters, and how cautious one ought to be in speaking positively of the presence of any organic substance. The plan they adopted was to consider what bland substances were most likely to be employed for the suspension of the turpentine, and then, by testing, to find with which the matter had most analogy. In this manner, after examining gum and milk, yolk of egg was found to resemble the residuum of the lotion more than any other substance, and no difference was found in their behaviour to tests. Thus, the residuum was found insoluble in water; with alcohol, a portion was soluble, and this, by evaporation, was found to be an oil quite similar to that from yolk of egg. The portion not soluble in alcohol or water, had all the appearance of the albumen which remains when yolk of egg is treated in a similar manner; and on being subject to destructive distillation, gave out abundance of ammonia, emitting a peculiar odour, precisely similar to that from burning egg. The ash that remained, after incineration, weighed three quarters of a grain, and consisted of a trace of alkali with phosphate of lime. On such evidence, which he admitted, however, could not be considered as perfectly conclusive, he proceeded to test it synthetically, and having prepared some according to the following prescription, he found it to resemble the quack lotion most perfectly in appearance, taste, and smell.

Take—The yolk of one egg; pure oil of turpentine, one ounce and a half; strong acetic acid, one ounce; pure water, three ounces. First rub the yolk of egg, the water, and the acetic acid together, then add the oil of turpentine, and agitate the whole till they be well mixed.

He had employed the liniment pretty extensively during the last month, and found the results consequent on its application, perfectly the same as from Long's. The phenomena which followed the use of either one or the other, which were deemed miraculous by the votaries of quackery, and

[1] Lond. Lancet, June 30.

paradoxical by many of the medical profession, were, in his opinion, easily explicable on pathological principles. They were,

First. The excitement of a greater degree of redness on one part of the surface than another when uniformly rubbed, this spot generally being in the neighbourhood of the part where pain was felt.

Secondly. On the continuation of the rubbing for some time, the oozing of a fluid from the surface, varying in colour, the cuticle still remaining unbroken.

Thirdly. The healing of the part under the continued action of the lotion.

These effects, he contended, were what one ought naturally to expect from any moderate stimulus when applied to the skin with dexterous manipulations. With respect to the first, it was admitted by all pathologists, that when inflammation terminated in abscess, or when a foreign substance was lodged in the body, the tendency of the pus, or the extraneous body, was to the surface. Of this there were constant instances, in warm climates, occurring in the liver. Now, long prior to any visible change in the surface, the skin over the diseased part, or in that point where the abscess is likely to show itself, is found to be more tender than the rest of the surface, as pressing the palm of the hand over it will show. It stands to reason, therefore, that this point will, when stimulated, be more easily affected, and hence a greater degree of redness will develope itself in that position. He did not see why we should restrict this tendency to the surface to cases of inflammation terminating in suppuration. A sympathy of the skin with all internal inflammation, as is seen in gout and rheumatism, might be assumed as a general principle, and of course the part sympathising the most will be the most easily affected by stimuli. As to the second phenomenon, the oozing out of a fluid from the surface, this was the necessary consequence of a stimulus not sufficiently powerful to cause vesication. When a very strong stimulus was applied to the skin, the cuticle appeared to lose its vitality; the mouths of the exhalents would not permit any fluid to pass through them, and hence the increased quantity flowing to the excited part became effused under the cuticle. Where, however, a gentle stimulus was for a length of time applied to the surface, the mouths of the exhalents, became stimulated without the cuticle losing its vitality or being abraded; the consequence was, there was a greater flow of fluid through the mouths of the exhalents. This, at first, would be little more than water containing some salts, but as the excitement was continued the vessels allowed a denser fluid to pass through them, consisting of the serum and crassamentum, without the red globules; but, should the excitement be very great and the part itself very much affected by the disease, the vessels often allow the red globules also to pass. That all these different fluids do exude, a few applications of any mild stimulus for half an hour over an irritable surface will be sufficient to convince the most sceptical. Touching the last effect, the curing of the part under the continuance of the rubbing, this, in St. Long's practice, was dependant partly on legerdemain, and partly on the principle that when a substance does not act as a corrodant, the surface will ultimately become accustomed to it; but the act of manipulating was the principal one. It has been seen that it is the continued rubbing, more than the lotion, which causes the flow of fluid from the surface; it is, therefore, quite natural, that if the rubbing be diminished in intensity, the stimulus becomes much less, and thus the part heals under the application of the same lotion, but differently applied. Such, therefore, is the celebrated liniment of St. John Long, which, in itself, is harmless, the acetic acid being to the whole in nearly the same proportion as in common vinegar, and which could only be dangerous in the hand of an ignorant and reckless quack.

He would take leave of the subject by mentioning, that when a discharge had been established a cabbage leaf was applied over the surface, and this,

in many respects, is superior to a common cataplasm, which is clumsy and dries up rapidly; but of course no regular practitioner would employ cabbage leaves while the simple and elegant contrivance, lint covered with oil silk, was within his reach.

On the Structure of Erectile Tissues.—In our number for January 6th, 1836,[1] we published a detailed account of the discoveries of Professor Müller in the minute structure of the penis, the substance of which was that there exists a separate series of minute arteries (which, from their form, he named *helicine*) projecting into the venous cells, and producing erection of the organ by the increased flow of blood through them, under circumstances of nervous excitement. His investigations were certainly of great importance, not only as establishing the minute structure of the organ described, but, in a general point of view, as relating to a distribution of vessels to which no similar arrangement had been presumed to exist in the animal body—viz. arteries terminating with free extremities, in cells communicating with veins. We have now, however, to notice a complete refutation of the views then advanced, and since almost universally received, which has been published in the last number of Müller's Archiv. für Anatomie und Physiologie. It is from the pen of Professor Valentin, the well-known author of the Entwickelungsgeschichte, whose talent for minute observation is certainly not inferior to that of Müller himself.

He says, that the result of numerous examinations has convinced him that the so-called helicine arteries are by no means peculiar vessels, terminating with their extremities, and hanging free in the cells of the corpus cavernosum, but only minute arteries that have been divided or torn; and that, on the contrary, the real distribution of the vessels of the corpus cavernosum follows in every respect the most simple laws. In making the injections of the penis, different portions of it receive different quantities of injection; in general the posterior half is most injected, and of this the anterior fourth is best adapted for examination, because in it the injection will probably have exactly filled the minute arteries without passing into the venous cells. If a transverse section of a portion thus injected be made, one sees on its surface, together with arteries of various sizes running tortuously in the uninjured fibrous cord-like partitions[2] of the cells of the corpus cavernosum, the *helicine arteries*—that is, arteries which, to the naked eye, or with a lens, seem to terminate suddenly, either singly or in tufts, which lie partly on the fibrous cords, but principally hang loose in the cells, and which, when placed in water, appear fixed at one end, while the other floats out in it. They all, even to the naked eye, appear completely inclosed by a membrane exactly like the tissue of the partitions; and if they are examined with a microscope, their ends appear sometimes rounded, sometimes obliquely or unevenly truncated, sometimes granular or even irregular; in a word, so inconstant is the form of their terminations, as at once to suggest the opinion that they are unnaturally formed.

If a cleanly cut transverse section be examined with a good lens, with which a view to some little depth may be obtained, it will at once appear that there are helicine arteries only at the surface, and in the cells lying near it; but that in those cells which lie deeper, no trace of them can be seen. At the same time it may be remarked, that every fibrous cord, without exception, contains an artery of proportionate size, which runs in it tortuously, or rather in the form of a cork-screw; and that these arteries, like the fibrous cords in which they lie, communicate together. If the surface of the section be examined under water, it will be seen that at the

[1] Lond. Med. Gaz., June 23.

[2] *Balken*, beams: the fibrous cords or bands which bound and traverse the so-called cells of the spleen.

divided extremity of each fibrous partition, one or more helicine arteries seem to be given off, according as one or more smaller fibrous cords are given off together or separately from the chief one. These smaller fibres, when they were divided, had separated and contracted a little, and thus, and by their naturally winding course, the tendril-like or crozier-like terminations of the supposed helicine arteries were produced. Thus one sees how the helicine arteries are formed under one's own eyes. And wherever the minute arteries are filled with injection they may be made to appear helicine by dividing the fibrous cords in which they lie. In a longitudinal section the same thing may be observed, only that here still more partitions being divided, more helicine arteries are seen; and more still may be made by cutting the corpus cavernosum, as one would with a saw; or by washing out the injection from the cells into which it has run from the arteries, and so tearing a greater number of the extremely minute cords. By the careful examination of several of the arteries, and their fibrous cords supporting them, which are thus divided, a sufficient proof may be obtained that the apparent enlargement of their extremities, the closeness of their orifices, and their tortuous or tendril-like course, depend merely on the mode in which the section has been made, or on some artificial means employed in the examination.

In the posterior part of the corpus cavernosum in man the cells are large, and the fibrous cords traversing them very delicate, so that as all the minute arteries run tortuously on them, the helicine arteries seem to be very abundantly and evidently present. But more anteriorly, where the cells assume a more honeycomb appearance, and the fibrous partitions are more band-like than cord-like, and the arteries running on them are proportionally much smaller than the membranes surrounding them, the helicine appearance cannot be demonstrated. The most easy refutation of the presence of the so-called helicine arteries is found in the human species (in which, it will be remembered, Müller said they could be most easily demonstrated,) and next to it, in that portion of the corpus spongiosum urethræ of the horse and ass which immediately surrounds the urethra.

Ice-cold Drinks and Cold Baths in Thoracic Inflammation.—In the year 1834, several cases were laid before the Academy of Naples, which were cured by the free use of snow internally, and of cold baths in every period of the disease. Recently M. Campagnano has published several similar cases.[2] In every case, however, bleeding appears to have been employed.

Proto-Iodide of Iron in Syphilis.—The proto-iodide of iron has been used with much advantage, externally as well as internally, in old cases of syphilis, and in those of a scrofulous or lymphatic temperament. It has been employed to remove the atony and bad character of certain ulcers, as well as chronic discharges from the urethra and vagina.

When given internally it may be combined with bitters, or alone, in the dose of from six grains to forty in the day. Externally, it is used in the form of lotions or injection, being mixed with an equal quantity of water and filtered.

The proto-iodide may be obtained by bringing into contact, when heated, a mixture of fifty parts of iodine, one hundred parts of pure water, and fifteen parts of iron filings. The solution deposites a greenish hue; it is

[1] Osservatori Med. di Napoli, Gazette Médicale de Paris, No. 7, 1838, and Zeitschrift für die gesammte Medicin. Mai, 1838. s. 82.

then filtered and evaporated rapidly in a large matrass, avoiding as much as possible the contact of atmospheric air. The residue must be kept in a well-stopped bottle.[1]

Library of Medicine.—In the late London advertisements there is one of a "Library of Medicine," to be conducted by Dr. Alexander Tweedie, one of the editors of the London Cyclopædia of Practical Medicine, with the assistance of several of the most eminent English writers. The object is to treat of each department or division of medicine in closely printed small octavo volumes, each series forming a complete work on the subject treated of.

We think it not improbable that this new undertaking may furnish some aliment for our own "Library."

Medical School of Yale College.—Professor Knight has been transferred to the Chair of Surgery in this institution, vacated by the death of Professor Hubbard; and Dr. Charles Hooker, of New Haven, of whom we have made favourable mention more than once in the "Intelligencer," has been appointed to the Chair of Anatomy and Physiology.

NECROLOGY.

Dr. Sims.—We regret to announce the decease of Dr. Sims, which took place at his house in Cavendish Square, of fever, and after a very short illness, on the 19th of July. Dr. Sims was one of the physicians to the St. Marylebone Infirmary, and a member of the senate of the London University, in which he took rather an active part; and where he was present at a meeting so lately as the 11th. He had contributed several papers of value to the Transactions of the Medical and Chirurgical Society; and had considerable business among the fraternity of Friends, of which he was himself a member.[2]

BOOKS RECEIVED.

From the Author.—Observations relative to Lymphatic Hearts. By Joseph Allison, M. D. (Extracted from the American Journal of the Medical Sciences, for 1838.) 8vo, pp. 19. Philadelphia, 1838.

Annual Circular of the Medical College of Louisiana, containing a prospectus for the session of 1838 and 1839. 8vo, pp. 6. New Orleans, 1838.

[1] Revue Médicale, Janvier, 1838.

[2] Lond. Med. Gaz., July 21, 1838.